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Lies, damned lies, and alibis: how do evaluators process alibis?

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Lies, damned lies, and alibis: How do evaluators process alibis?

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ABSTRACT

Research regarding the evaluation of alibis has been sparse to date. The current experiments were designed to assess how people evaluate and process alibi statements. It was hypothesized that people approach alibis with skepticism and that alibis elicit different processing from evaluators than do everyday memory statements. Experiment 1 used a cognitive busyness paradigm to assess the starting point from which alibi evaluators begin. Results indicated the cognitive busyness manipulation did not affect participants' ratings of believability of either the weak or strong alibi. The superficial evaluation hypothesis was advanced as a potential explanation for the ineffectiveness of the busyness manipulation—that participants who were cognitively busy were interrupted in their processing of the central facts of the case but were nonetheless able to use simple, peripheral cues to arrive at similar evaluations as non-cognitively-busy participants. Experiment 2 manipulated the timing and type of alibi schema to examine whether alibis are processed differently from a normal narrative story. All participants viewed the same narrative account; some participants knew prior to viewing that the account was an alibi whereas others discovered after viewing that the account was an alibi. In addition, some participants were told the guilt or innocence status of the alibi provider. Results indicated that when participants did not know the ultimate status of the alibi provider and knew that they were viewing an alibi prior to watching the alibi video, their recall was biased toward details occurring during the time period most relevant to the alibi. Knowledge that the account was an alibi affected participants' encoding, but not retrieval, of the alibi story. There was no clear support for the hypothesis that alibi evaluators approach alibis with skepticism, but there was support for the hypothesis that people encode alibi information differently than they encode an everyday narrative account.

INTRODUCTION

On September 11, 2003, Steven Avery walked out of prison. Seventeen years after he was convicted of sexual assault and attempted murder, Avery was the 137th American man exonerated by post-conviction DNA testing (The Innocence Project, n.d.). Avery's case highlights the difficulty of presenting an alibi defense that evaluators would find compelling: He presented 16 alibi corroborators who, together, established his whereabouts for the entire day. Some of these corroborators were family and friends; others were strangers—store clerks who presented receipts and testimony that they remembered him because they thought it was unusual that he was shopping with his wife and five children, including 6-day-old twins. An alibi is theoretically enough to keep an innocent person out of trouble: off the suspect list, out of the courtroom, out of prison. So why was such a compelling alibi not enough to keep Avery out of prison?

How does a person go about evaluating an alibi, weighing its believability, and deciding whether the alibi provider is truthful or not? Does the alibi evaluation process differ from other types of evaluation processes? Some initial evidence suggests an alibi skepticism hypothesis—that evaluators are particularly skeptical with regard to alibis. In a study designed to test a classification system for scoring alibis, Olson and Wells (2004a) presented alibis with varying types of corroboration to participants and asked them to rate how believable they found the alibis. The alibis factorially combined physical evidence corroboration and person evidence corroboration. The physical evidence included no evidence, easily fabricated evidence in the form of a cash receipt, and difficult to fabricate evidence in the form of a security camera video. The person evidence included no corroboration, a motivated familiar other corroborator in the form of a close relative or

friend, a non-motivated familiar other in the form of a regular waitress or store clerk, and a non-motivated stranger, who had no previous relationship with the alibi provider, in the form of a taxi driver or store clerk. Olson and Wells found that participants rated the alibis that combined no person and no physical evidence as least believable (mean 4.8 on a 0-10 scale) and alibis that combined a non-motivated familiar other and difficult-to-fabricate physical evidence as the most believable (mean 7.4 on a 0-10 scale). Olson and Wells note that even the most compelling alibis, with videotape corroboration, were still not regarded as wholly believable by their participants.

What kind of evidence might be needed for an alibi to earn a score of 10? The study by Olson and Wells has its limitations—they asked participants to take the role of a detective on a case, and this role-playing may have given participants a more skeptical mindset. The alibis used were written descriptions of the alibis given by the suspects in the case—perhaps a richer context, in which participants see the alibi provider on video, might reduce the skepticism exhibited by participants in the Olson and Wells study. Nevertheless, the Olson and Wells study provides some initial empirical evidence of alibi evaluators' skepticism.

The post-conviction DNA exoneration cases also provide some anecdotal evidence that suggests support for the alibi skepticism hypothesis. Steven Avery's case is unusual in its number and variation of alibi corroborators; however, it is important to remember that in all the DNA exoneration cases, the innocent men had alibis that were likely true but were nonetheless ineffective. Forty DNA exoneration cases highlighted in a position paper of the American Psychology-Law Society (Wells et al., 1998) occasionally included a weak alibi or no alibi as *incriminating* evidence. However, the convictions in these cases were likely not due only to evaluators' skepticism about alibis but were likely the result of a number of

contributing factors. For instance, the DNA exoneration cases show a high rate of mistaken eyewitness identifications. A confident eyewitness identification, even a mistaken one, is a powerful piece of incriminatory evidence (Lindsay, Wells, & Rumpel, 1981; Wells, Lindsay, & Ferguson, 1979). Hence, the convictions in these DNA exoneration cases might simply reflect the persuasive power of the incriminating (albeit incorrect) evidence rather than reflect the weak persuasive power of the alibis.

Thus far, the evidence that people approach the evaluation of alibis with skepticism is slim, but findings in other areas of social psychology support the idea that alibis might arouse skepticism. For instance, from the persuasion and social influence literature, the credibility of a speaker varies according to his or her *expertise* and *trustworthiness* (Hovland, Janis, & Kelley, 1953). An evaluator is not likely to question an alibi provider's expertise—it is reasonable to expect that a person know where he or she was at a given time. However, an alibi provider's trustworthiness is central to an evaluation of an alibi. A speaker is perceived as more credible and trustworthy when he or she attempts to persuade on a topic that is seen as something not in the speaker's self-interest (Eagly, Wood, & Chaiken, 1978; Walster, Aronson, & Abrahams, 1966). Alibi providers, by the very nature of the task they must perform, are speaking in their own self-interest, trying to keep out of trouble. Thus, the alibi provider (and by extension the story) may be seen as considerably less credible than if he or she had told a story that was not an alibi.

Attribution theory gives us a related clue as to why evaluators might approach alibis with skepticism. Kelley (1973) hypothesized that a given explanation for a behavior is given less weight (discounted) if additional explanations are present or can be imagined. Wells and Ronis (1982) found that the number of additional explanations for a behavior are less

important to an evaluator's attribution than the total strength of the additional explanations. Discounting of a given explanation also tends to be greatest when the explanations are mutually exclusive (McClure, 1998). In the alibi situation, one possible explanation is that the alibi provider is giving a factual account of his or her whereabouts. However, a salient, and mutually exclusive, alternative explanation is the "alibi provider is lying" explanation—that the alibi provider is concealing his or her role in the crime and is trying to "get away with it." Perhaps the "alibi provider is lying" explanation is strong enough that the evaluator discounts the believability ascribed to even a true alibi.

Wegner, Wenzlaff, Kerker, and Beattie (1981) identified the innuendo effect—another reason to expect that evaluators would regard alibis with suspicion. Innuendo is a potentially damaging statement about a person and a qualifier which denies the truth of the original statement. In Wegner and colleagues' research, an innuendo often took the form of a newspaper headline in the form of a question or a denial, such as "Andrew Winters Not Involved in Bank Embezzlement." One would expect that the original, damaging statement would not affect judgments of the target person—the qualifier should negate the original statement. However, Wegner and colleagues found that the original statement retains a considerable influence on evaluators in spite of the qualifier. The original statement is generally more salient to the evaluator, and thus better remembered, than the qualifier. Wegner and colleagues state that they would expect to find innuendo effects occurring in situations similar to the context of their study: a highly charged atmosphere of evaluation and a lack of prior audience knowledge of the target, which sounds quite similar to the alibi situation.

The social understanding framework (see Ybarra, Schaberg, & Keiper, 1999) outlines how evaluators' expectancies about a target person can shape perceptions and judgments of that person. Social norms tend to encourage positive behavior, and targets who exhibit positive behavior can be perceived as conforming to social pressures—their behavior is attributable to the situation. Negative behavior, on the other hand, breaks powerful situational social norms; thus, evaluators perceive negative behavior as more indicative of the target's disposition. Rothbart and Park (1986) showed that very little negative information is needed for an evaluator to infer that a target is unfavorable. Perhaps the mere knowledge that a person is providing an alibi is enough negative information to arouse unfavorable impressions of the alibi provider and thoughts of suspicion.

Evaluators may also operate under the assumption that a truthful alibi ought to have a greater level of proof than the average person can provide. Olson and Wells (2004b) gave participants an alibi with a moderate level of corroborating evidence (the suspect claimed to be watching a football game with his friend, who corroborated the story). Half the participants were asked to generate an alibi of their own prior to evaluating the alibi, the other half were asked to evaluate the alibi and then generate an alibi of their own. The participants who were asked to generate their own alibi first rated the suspect's alibi as more believable than those who were asked to evaluate the suspect's alibi first. Perhaps those who generated their own alibi first discovered how difficult it was to come up with an alibi story and thus gave the suspect the benefit of the doubt when evaluating his alibi.

Although there are theoretical reasons to think that alibi evaluators are highly skeptical, there are no data directly addressing this alibi skepticism hypothesis. Furthermore, the psychological processes that might characterize this skepticism have not been studied.

The following studies attempted to address both of these questions by using methods that assess the "starting point" that alibi evaluators use and that assess how alibi information is encoded and retrieved from memory. Experiment 1 used cognitive busyness procedures and belief measures to examine the starting point from which alibi evaluators begin processing. Where might skepticism enter the process—does the evaluator begin a skeptic or does disbelief enter later in the process? Experiment 2 used memory measures to examine the cognitive processes of alibi evaluators. Do evaluators tend to organize the storyline of an alibi in a different fashion than they would organize an average autobiographical account? Is this organization reflected in the evaluators' encoding of the story, retrieval of the story, or both?

EXPERIMENT 1: USING COGNITIVE BUSYNESS TO INFER ALIBI EVALUATORS' STARTING POINT

One useful way to think about the initiation of the alibi evaluation process comes from the judgment and decision-making literature—the starting point can be thought of as an anchor from which evaluators must adjust to reach their final decisions. Anchoring and adjustment is a well-known heuristic first described by Tversky and Kahneman (1974). A decision maker uses an initial value as an anchor and adjusts his or her estimate away from that anchor, using other information he or she may have. However, if the decision maker fails to adequately adjust from the initial anchor, biased judgment may result. Gilbert (2002) argued that "anchoring and adjustment describes the process by which the human mind does virtually all its inferential work" (p. 167).

Alibi evaluators have several possible anchors or starting points when they first begin evaluating an alibi. They could begin with an assumption of truth; their starting point would be belief in the alibi. They could begin with an assumption of neutrality, neither inclined to believe the alibi provider's story nor disbelieve it. Lastly, they could begin with an assumption of deception, assuming that the alibi provider is actually guilty of the crime and is lying when telling his or her story; their starting point would be skepticism of the alibi. I will address theoretical and empirical support for why one might expect each of these starting points.

One possible starting point for alibi evaluators is the assumption of truth, assuming that the alibi provider is telling the truth and the alibi is accurate. The assumption of truth is fundamental to everyday discourse. According to Grice (1975), conversation is a cooperative effort governed by several principles and sub-principles. One of these maxims is quality—

participants in a conversation should not say things that they believe to be false or for which they lack sufficient evidence. Grice (1975) pointed out that the maxim of quality holds even in interactions that are not conversational in nature—when making a cake and the recipe calls for sugar, I do not expect my sister to hand me salt. O'Sullivan noted that people generally tend to exhibit an assumption of truth and judge others as truthful most of the time (2003; see also O'Sullivan, Eckman, & Friesen, 1988). Gilbert (1991) argued that a default assumption of truth is adaptive, that we must believe our senses and other people in order to survive—when someone yells, "Watch out for that tree!" I do not have time to entertain the possibility the warning is false; I must react immediately.

The philosopher Baruch Spinoza argued that a default assumption of truth is a fundamental component of the way the human mind understands and believes ideas. He proposed that in the instant the mind comprehends an idea, it must implicitly believe the idea to be true, that comprehension and belief are one and the same process (Spinoza, 1677/2000). However, people are not destined to believe every idea they have ever had; Spinoza thought that people could undergo a second processing step and evaluate an idea by comparing it with existing knowledge. If the existing knowledge contradicts the new idea, the new idea is thrown into doubt and it may be tagged false. A key element of a Spinozan system is that if a person is prevented from comparing a new bit of false information with existing knowledge (i.e., prevented from thinking about the false information), then the person should act as if the false information is true. The Spinozan position thus predicts that alibi evaluators should begin with an assumption of truth.

Spinoza's conception of the psychology of belief and the starting point that people operate from contradicts the conception of belief put forward by his contemporary René

Descartes, whose philosophy was influenced by his strong commitment to the idea of free will. Descartes stated that comprehension and perception are one and the same—that people comprehend ideas passively and effortlessly; however, labeling an idea true or false (i.e. believing or disbelieving the idea) is something that requires willful action by the mind (Descartes, 1644/1985). Unlike a Spinozan processor, if a Cartesian system is prevented from fully processing a new bit of false information, then the person should be no more likely to act as if the false information is true than he or she would be to act as if the false information is false. Essentially, Descartes argued that people do not have a default assumption of truth; indeed, Descartes' philosophy states that people do not have a default assumption at all. The Cartesian position thus predicts that alibi evaluators would begin with an assumption of neutrality.

Because the essential difference between Spinozan and Cartesian belief systems is their starting point, it is useful to examine how one group of psychologists has set about identifying which system (and starting point) people really have. Through several studies (Gilbert, Krull, & Malone, 1990; Gilbert, Tafarodi, & Malone, 1993) Gilbert and colleagues used the cognitive busyness paradigm to prevent participants' processing from being completed and to force premature output of their evaluations. In this paradigm, participants are given information to remember, some of it true and some of it false. For example, in the 'Hopi language experiment,' participants see phrases defining 'Hopi' words (which are actually nonsense words). Shortly after the phrase is presented, they learn either that the phrase is true or false. Cognitively busy participants are given an additional task to perform while they are attempting to learn the 'Hopi' words. Later they are asked to recall the phrases and identify each word and its definition as true or false. Gilbert and colleagues reasoned that

if the mind works in a Spinozan fashion, then making participants cognitively busy would allow them to comprehend a statement (which involves believing the idea to be true) but should prevent them from undergoing the second step of disbelieving an idea, or tagging the idea as false. According to this reasoning, cognitively busy Spinozan processors should err in the direction of identifying the false phrases as true. In contrast, if the mind works in a Cartesian fashion, there is no reason to hypothesize any directional pattern of errors. Gilbert and colleagues showed consistently that people are Spinozan processors—without sufficient resources to disbelieve an idea, their participants tended to behave as if the false idea were true. In the case of alibi evaluators, then, it makes sense to hypothesize that their starting point reflects the assumption of truth—they believe the alibi provider's story is true, and only later in the evaluation process do they integrate their knowledge that the story is an alibi and alter their judgment to be more skeptical.

Alternatively, skepticism could enter alibi judgments early in the processing of an alibi. If alibi evaluators assume that the alibi provider is actually guilty of the crime and is lying when telling his or her story, evaluators would begin with an assumption of deception. An assumption of deception regarding the alibi provider is not implausible; Cacioppo and Berntson (1994) noted that if a source of a message is known to be a liar, statements from that source are not assumed to be true by recipients. A possible explanation for the assumption of deception comes from Skurnik, Moskowitz, and Johnson (2003), who take issue with Gilbert's (1991, 1993) explanation of how people might come to erroneously report that false information is true. According to Skurnik et al.'s framework, when people are uncertain about the source of a given piece of information (when they are unsure if it is true or false), they use preexisting knowledge and general beliefs about the likelihood that

the information is true to determine if the information is true or false. Skurnik et al. (2003) were able to manipulate participants' general likelihood beliefs about the prevalence of true and false information in their experimental sessions. They did this by having participants learn phrases as in Gilbert et al.'s (1990) 'Hopi language experiment' during an initial session of the experiment. Participants in mostly-true conditions saw 2/3 true phrases and 1/3 false phrases, whereas participants in mostly-false conditions saw 1/3 true phrases and 2/3 false phrases. Participants were asked to recall the phrases and identify them as true or false. After the initial learning/recall session, participants began a second (and ostensibly unrelated) learning session of phrases. In the second session, some participants were made cognitively busy (by listening for sequences of odd numbers over headphones) while attempting to learn the phrases. Note that the second session consisted of ½ true phrases and ½ false phrases. When asked to identify the true and false items they had studied, cognitively-busy participants' judgments reflected their general likelihood beliefs about the prevalence of true items: Similar to Gilbert and colleagues' findings, participants who had previously encountered mostly true information mistakenly judged more false items as true. However, participants who had previously encountered mostly false information made the opposite mistake—they mistakenly judged more true items as false. If alibi evaluators have and use a general belief that alibi providers are deceptive, they would begin from a starting point of disbelief in the alibi and may insufficiently adjust their belief upon hearing a strong alibi.

The cognitive busyness paradigm was used by Gilbert and Skurnik and their colleagues to investigate a default setting of the human processing system: in their case, belief in an idea. I used the cognitive busyness paradigm to examine a similar problem: namely, the starting point of people who attempt to evaluate an alibi. By preventing

evaluators from undergoing extensive processing of the alibi and then asking them to evaluate the alibi, I hoped to gain some insight into the starting point from which evaluators begin. I predicted that non-cognitively-busy participants should exhibit a similar pattern of believability ratings as the participants in Olson and Wells (2004a): They would judge the weak alibi as less believable than the strong alibi. However, the cognitively-busy participants would be prevented from thoroughly processing the alibi and should be unable to revise their initial beliefs about the alibi. I hypothesized that cognitively-busy participants start from an initial assumption of deception; thus, cognitive busyness should produce little or no reduction in belief ratings of the weak alibi, but busyness should primarily serve to lower belief ratings for the strong alibi.

Method

Participants. Participants were 184 undergraduate students from a large Midwestern university recruited for an experiment entitled "The Psychology of Belief." Participants earned extra credit in psychology classes for their participation. All participants were treated in accordance with the "Ethical Principles of Psychologists and Code of Conduct" (American Psychological Association, 2002).

Design. The design for Experiment 1 was a 2 x 2 between-subjects design: Participants were randomly assigned to be presented with an alibi story supported by either weak evidence or strong evidence. Participants were also randomly assigned to be either cognitively busy (with a visual search task) or not cognitively busy (with no additional task).

Stimulus Materials. Participants watched one of two videos: A person giving either a weak alibi or a strong alibi. The alibi provider in the video was a 25-year-old Caucasian male seated in front of a white wall, talking to someone off-camera. The videotaped alibis

followed the same script, except for statements made by the off-camera investigator. In the weak alibi video, the investigator interrupted the alibi provider to state that his sister was uncertain of the day she saw him, and again to state that the clerk at the store did not remember him specifically (see Appendix A for the script). In the strong alibi video, the investigator interrupted the alibi provider to state that his sister was very certain of the day and time she saw him, and again to state that the purchase he claimed to have made was on his credit card statement (see Appendix B for the script). Pretesting of the videos indicated that participants rated the strong alibi as stronger than the weak alibi (mean ratings 4.15 and 3.38 on a 7-point scale, respectively; $t(42) = 2.07$, $SE = 0.37$, $p < .05$). Each video was approximately four minutes long, presented in a 4" wide x 3" high window on a computer screen.

A scrolling number line appeared below the video. The line consisted of random single digits, spaced approximately ½-inch apart, scrolling at a moderate speed (approximately one new digit every second) from below the right corner of the video window to below the left corner of the video window. Sequences of three odd digits appeared at an average rate of one sequence every 12 seconds. For the weak alibi video, a total of 21 sequences appeared; for the strong alibi video, 18 sequences appeared. The slight difference in the number of odd-digit sequences is due to differences in the amount of time the investigator spoke during her interruptions, making the weak alibi video slightly longer than the strong alibi video.

Procedure. Approximately half the participants ($n = 91$) viewed the videotaped alibi with weak supporting evidence, and half ($n = 93$) viewed the videotaped alibi with strong supporting evidence. Participants in the cognitively-busy conditions were asked to search for

sequences of three odd numbers in the number line and to mentally note how many sequences they saw. Participants in the not-cognitively-busy conditions were asked to ignore the number line. When the video finished, participants were instructed, via computer screen, to answer all questions as quickly as possible. Questions were then presented via a computer program. When participants finished the questionnaires, they were debriefed by the experimenter, thanked, and dismissed.

Dependent Measures. For the primary dependent measure, immediately after watching the alibi video participants rated the alibi on an 11-point Likert-type scale of believability (0 = *I do not believe him at all*, 10 = *I believe him completely*; hereafter called the *belief* measure). Participants were also asked to rate on an 11-point Likert-type scale how believable they thought a detective would find the alibi (0 = *not at all*, 10 = *completely believable*; hereafter called the *detective belief* measure). Participants then rated how strong they thought the person's alibi was on a 7-point Likert-type scale (1= *very weak*, 7 = *very strong*; hereafter called the *alibi strength rating*).

Participants also rated the alibi provider on how much he was described by each of the 20 traits first used by Olson and Wells (2004a). Twelve of these traits were considered to be relevant to the believability of the alibi provider (i.e., *calculating, conniving, cunning, deceitful, honest, intelligent, loyal, open, scheming, sincere, suspicious, trustworthy*); these were significantly correlated with believability in Olson and Wells (2004a). Eight were considered to be irrelevant to the believability of the alibi provider (i.e., *ambitious, caring, content, curious, friendly, funny, shrewd, shy*); these were not significantly correlated with believability in the Olson and Wells (2004a) study. For ease of analysis, I followed these trait

divisions in the current study. Traits were rated on a 7-point Likert-type scale (1 = *does not describe this suspect at all*, 7 = *describes this suspect perfectly*).

Lastly, participants in the cognitively-busy conditions were asked how many sequences of three odd digits they counted. All participants were asked two manipulation-check questions to determine if they heard and understood the information in the detective's interruptions that made the alibi either strong or weak. The first question asked, "When the detective said she talked to the man's sister, what was the sister's reaction?" Participants could answer "A: The sister was pretty sure she had called her brother for help that Saturday" or "B: The sister was quite certain her brother fixed her car that Saturday." For the weak alibi video, option A is correct and for the strong alibi video, option B is correct. The second question asked, "What did the detective say regarding the man's visit to the NAPA store?" Participants could answer "A: The man's purchase was listed on his credit card records" or "B: The clerk at NAPA remembered the purchase and did not recognize the man's picture." For the weak alibi video, option B is correct and for the strong alibi video, option A is correct.

Results

The Belief Measure. Participants rated "How much do you believe this person's alibi?" on an 11-point Likert-type scale. Means and standard deviations can be seen in Table 1. For effect sizes, Cohen's f is reported, and a small effect is 0.15, a medium effect is 0.25, and a large effect is 0.40 (Rosenthal & Rosnow, 1991). There was no interaction between cognitive busyness and alibi strength $F(1, 180) = 0.74, p > .05, MSE = 4.22$, Cohen's $f =$ 0.06. Likewise, there was no main effect for cognitive busyness $F(1, 180) = 0.38, p > .05$, Cohen's $f = 0.05$. However, there was a main effect for alibi strength: participants rated the

strong alibi as more believable than the weak alibi $F(1, 180) = 28.61, p < .05$, Cohen's $f =$ 0.40.

Table 1. Mean belief measure ratings as a function of cognitive busyness and alibi strength

Belief Rating (0 = *I do not believe him at all*, 10 = *I believe him completely*)

	Cognitive Busyness		
Alibi Strength	Busy	Not Busy	Total
Weak	5.50 (2.22)	5.57 (1.83)	5.54 (2.01)
Strong	7.38 (1.96)	6.93 (2.21)	7.16 (2.08)
Total	6.47 (2.28)	6.25 (2.12)	

Detective Belief Rating (0 = *not at all*, 10 = *completely believable*)

	Cognitive Busyness		
Alibi Strength	Busy	Not Busy	Total
Weak	4.80 (2.09)	4.38 (1.85)	4.58 (1.97)
Strong	6.66 (1.75)	6.37 (2.47)	6.52 (2.13)
Total	5.76 (2.13)	5.37 (2.39)	

Alibi Strength Rating (1 = *very weak*, 7 = *very strong*)

	Cognitive Busyness		
Alibi Strength	Busy	Not Busy	Total
Weak	3.47 (1.28)	3.23 (1.40)	3.35 (1.34)
Strong	4.66 (1.29)	4.52 (1.46)	4.59 (1.37)
Total	4.09 (1.41)	3.87 (1.56)	

Note: Standard deviations in parentheses

The Detective Belief Measure. Participants rated "How believable do you think a detective would find this alibi?" on an 11-point Likert-type scale. Means and standard deviations can be seen in Table 1. There was no interaction between cognitive busyness and alibi strength $F(1, 180) = 0.04, p > .05, MSE = 4.22$, Cohen's $f = 0.02$. Likewise, there was no main effect for cognitive busyness $F(1, 180) = 1.34, p > .05$, Cohen's $f = 0.09$. However, there was a main effect for alibi strength: participants thought a detective would find the strong alibi more believable than the weak alibi $F(1, 180) = 40.38, p < .05$, Cohen's $f = 0.47$.

The Alibi Strength Rating. Participants rated "How strong do you think this person's alibi was?" on a 7-point Likert-type scale. Means and standard deviations can be seen in Table 1. There was no interaction between cognitive busyness and alibi strength $F(1, 180) = 0.07, p > .05, MSE = 1.85$, Cohen's $f = 0.02$. Likewise, there was no main effect for cognitive busyness $F(1, 180) = 0.90, p > .05$, Cohen's $f = 0.07$. However, there was a main effect for alibi strength such that the strong alibi was rated as stronger than the weak alibi $F(1, 180) = 37.85, p < .05$, Cohen's $f = 0.46$.

The Trait Ratings. The trait ratings were divided into the two categories, relevant traits and irrelevant traits, used by Olson and Wells (2004a). Correlations among the individual trait ratings and the three main measures can be seen in Table 2. Positive traits (*honest, open, sincere, trustworthy, loyal, intelligent, content, caring, curious, friendly, ambitious, funny*) were reverse-coded so that all scores carried a negative connotation (i.e., *trustworthy* became *untrustworthy*). Traits were then averaged to yield composite scores for both relevant and irrelevant traits (see Table 3 for means and standard deviations; Table 4 for correlations with the belief measures). There was no interaction between cognitive busyness, alibi strength, and type of trait $F(1, 180) = 0.01, MSE = 0.25, p > .05$, Cohen's $f = 0$.

Likewise, there was no main effect of cognitive busyness on participants' trait ratings $F(1, 180) = 1.57, p > .05$, Cohen's $f = 0.09$. However, as seen in the main measures, there was a main effect of alibi strength on participants' ratings, such that participants rated the alibi provider who gave the strong alibi more favorably than the alibi provider who gave the weak alibi $F(1, 180) = 33.02, p < .05$, Cohen's $f = 0.43$.

Table 2. Correlations among belief measures and individual trait ratings

	Main Measures		
	Belief	Detective Belief	Alibi Strength
Relevant Traits			
Conniving	-0.05	-0.13	-0.08
Calculating	0.02	0.04	-0.01
Deceitful	-0.27*	-0.34*	-0.35*
Suspicious	-0.56*	-0.53*	-0.45*
Cunning	-0.21*	-0.25*	-0.30*
Scheming	-0.36*	-0.45*	-0.39*
Honest	0.66*	0.67*	0.70*
Open	0.46*	0.45*	0.44*
Sincere	0.52*	0.52*	0.59*
Trustworthy	0.50*	0.59*	0.58*
Loyal	0.47*	0.53*	0.56*
Intelligent	0.40*	0.43*	0.43*
Irrelevant Traits			
Shrewd	-0.17*	-0.24*	-0.25*
Shy	-0.06	-0.11	0.02
Content	0.37*	0.35*	0.32*
Caring	0.25*	0.28*	0.32*
Curious	0.00	0.07	0.05
Friendly	0.31*	0.33*	0.30*
Ambitious	0.25*	0.26*	0.29*
Funny	0.30*	0.36*	0.27*

Note: Correlations marked with * are significant at $p < .05$.

Table 3. Mean composite trait ratings as a function of cognitive busyness and alibi strength

Relevant Traits

	Cognitive Busyness		
Alibi Strength	Busy	Not Busy	Total
Weak	4.17 (0.65)	4.45 (0.70)	4.31 (0.69)
Strong	3.74 (0.86)	3.71 (0.82)	3.73 (0.84)
Total	3.95 (0.79)	4.08 (0.84)	

Irrelevant Traits

	Cognitive Busyness		
Alibi Strength	Busy	Not Busy	Total
Weak	4.23 (0.62)	4.51 (0.73)	4.38 (0.69)
Strong	3.88 (0.80)	3.83 (0.68)	3.85 (0.74)
Total	4.05 (0.73)	4.17 (0.78)	

Note: Standard deviations are given in parentheses. All traits coded to have a negative connotation; higher composite scores mean a more negative evaluation.

Table 4. Correlations among composite trait ratings and belief measures as a function of cognitive busyness

Belief Rating

	Cognitive Busyness	
Traits	Busy	Not Busy
Relevant	-0.65*	-0.57*
Irrelevant[1]	-0.56*	-0.24*

Detective Belief Rating

	Cognitive Busyness	
Traits	Busy	Not Busy
Relevant	-0.54*	-0.39*
Irrelevant	-0.63*	-0.69*

Alibi Strength Rating

	Cognitive Busyness	
Traits	Busy	Not Busy
Relevant	-0.48*	-0.35*
Irrelevant	-0.61*	-0.72*

Note: Correlations marked with a (*) are significant, $p < .05$.

[1.] Correlations across rows significantly different from one another, $p < .05$.

Manipulation Check Measures. Participants in the cognitively-busy conditions were asked to report how many sequences of three odd digits they counted. Two participants in the

weak alibi video condition reported an extreme number of sequences (81 and 115) and were removed from the analysis.[1] A boxplot of the distribution of remaining reports may be seen in Figure 1. Participants reported counting a mean of 2.88 (*SD* = 6.64) fewer sequences than actually appeared in the weak alibi video and a mean of 2.32 (*SD* = 5.73) fewer sequences than actually appeared in the strong alibi video. These difference scores did not significantly differ from each other $t(87) = 0.43$, $SE = 1.31$, $p > .05$.

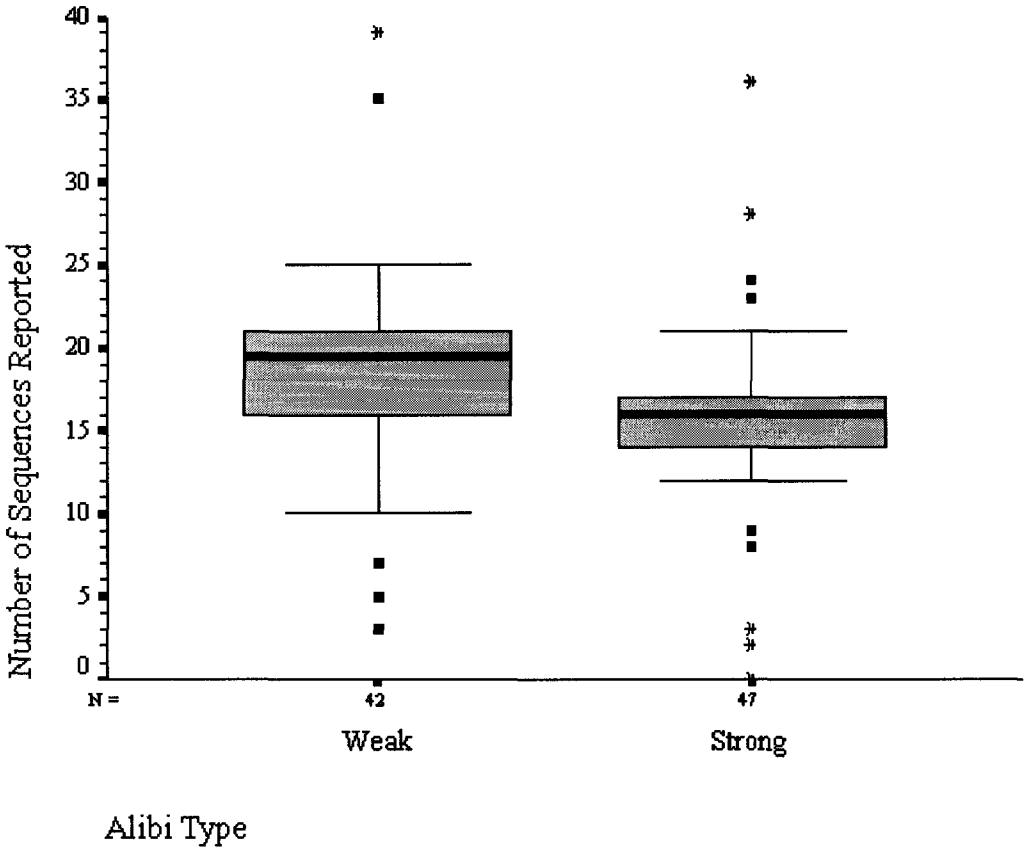

Figure 1. Distribution of Number of Sequences Reported as a Function of Alibi Strength

[1] Examination of other responses by these two participants did not indicate unusual responses to any of the other measures. It is likely that these extreme responses were the result of keying error by the participants, i.e. that a participant intended to answer "11" or "15" but instead typed a "115." With no way to know their true intention, I excluded these two participants from the sequence analysis.

Participants were also asked two multiple-choice manipulation check questions regarding the detective's interruptions in the video. Participants could respond to each question by choosing one of two options, one of which was correct for the weak alibi video and one of which was correct for the strong alibi video. In the weak alibi video condition, all participants answered both questions correctly. In the strong alibi video condition, 2 cognitively-busy participants and 6 non-cognitively-busy participants answered one of the two questions incorrectly while the remaining 43 participants answered both questions correctly.

Lastly, I examined the time which participants took to answer the first question following the video, the *belief* measure. Participants had been instructed to answer the questions following the video as quickly as possible and were also required to keep in mind the number of three-odd-digit sequences they had counted, so that they might continue to be cognitively busy while answering the main measures. However, if participants took a considerable amount of time to answer the initial questions following the video, they might have been able to negate the effects of cognitive busyness by thinking deeply about the video. Participants in the cognitively-busy conditions took an average of 6.95 seconds (SD = 4.62) to answer the *belief* measure, whereas participants in the not-cognitively-busy conditions took an average of 6.65 seconds (SD = 3.62) to answer the *belief* measure. There was no effect of cognitive busyness on time taken to answer the *belief* measure $F(1, 180) = 0.04$, $MSE = 0.19$, $p > .05$.

Discussion

As mentioned earlier, alibi evaluators may begin with any of several starting points. I hypothesized that evaluators, because of the power of the concept of 'alibi,' would begin

from an assumption of deception, assuming that the alibi provider is lying and is guilty of the crime. In the case of a weak alibi, evaluators' judgments of belief in the alibi would not need to be revised very much; however, in the case of a strong alibi, judgments of belief would be considerably higher than the initial assumption. In the event that evaluators are prevented from fully considering evidence (in the current experiment, by cognitive busyness), I would expect that judgments of belief in both strong and weak alibis to be very similar. Thus I would expect an interaction effect: in the absence of busyness, participants should rate strong alibis as more believable than weak alibis; cognitive busyness should produce little or no reduction in belief ratings for weak alibis but would primarily lower belief ratings for strong alibis, resulting in a smaller difference in ratings between weak and strong alibis in the busy conditions than the non-busy conditions. This is not consistent with the data; instead, the data show a main effect of alibi strength, with no main effect of cognitive busyness and no interaction.

An alternative hypothesis was that alibi evaluators could begin from an assumption of truth, assuming the alibi provider is telling the truth and is not guilty of the crime. From an assumption of truth, evaluators' judgments of a strong alibi would not need to be revised very much, but judgments of a weak alibi would rate the alibi as less believable than the initial assumption. In this case, I would also expect an interaction effect: in the absence of busyness, participants should rate strong alibis as more believable than weak alibis; cognitive busyness should produce little or no reduction in belief ratings for strong alibis but would primarily raise belief ratings for weak alibis, resulting in a smaller difference in ratings between weak and strong alibis in the busy conditions than the non-busy conditions. This is also not consistent with the data, which show only a main effect of alibi strength.

One might be tempted to conclude that if participants do not seem to be beginning with an assumption of deception or an assumption of truth, then they must be adopting an assumption of neutrality, assuming that the alibi is neither untrue nor true. However, the data do not support this conclusion, either; for an assumption of neutrality I would also expect to see an interaction effect. In the absence of busyness, participants should rate strong alibis as more believable than weak alibis; cognitive busyness should produce a reduction in belief ratings for strong alibis and an increase in belief ratings for weak alibis, resulting in a smaller difference in ratings between weak and strong alibis in the busy conditions than the non-busy conditions. The data do not show this pattern.

So what was really happening here? One possibility, of course, is that the cognitive busyness manipulation did not adequately tax participants, and they were able to fully process the alibi at the same level as non-cognitively-busy participants. However, I do not believe that this is the case. The task was modeled after a cognitive-busyness task used in Gilbert et al. (1993) in which participants scanned a number line for single digits. Informal pretesting and tweaking of the task for the current experiment with several participants and research assistants resulted in a sequence-searching task that was difficult to perform while processing the alibi, but was not so taxing as to destroy participants' ability to attend to the alibi story while keeping a reasonably accurate count in the sequence-search task. I believe the busyness manipulation did work insofar as it disrupted careful scrutiny of the alibi story.

One of the assumptions on which my original hypotheses rested was that the evaluation of alibis requires careful scrutiny, and that without this careful scrutiny, the starting point from which alibi evaluators begin would more strongly influence the final judgments of the evaluators. However, careful scrutiny might not have been necessary for the

evaluation of the alibi in the experiment. For example, cognitively-busy participants may have been able to arrive at similar judgments as non-cognitively-busy participants because the videotaped alibi used in this experiment did much or most of the inferential work for them. The videotaped alibi detailed a linear story with a script many college students would be familiar with (trying and failing to get homework done on a Saturday), and the alibi evidence was presented in a straightforward fashion by the detective in the video. Perhaps inferences about the strength of the alibi were easily made once participants comprehended the alibi—because the alibi was not complex enough.

Alternatively, cognitively-busy participants may have been able to arrive at similar judgments as non-cognitively-busy participants by using simple cues rather than the deeper level of evidence offered in support of the alibi. This research assumed that alibi evaluators adjust from their initial assumptions through an effortful consideration of the evidence and story of the alibi, i.e., centrally-processing the alibi, to borrow from the elaboration-likelihood model of persuasion (Petty & Cacioppo, 1986). It is possible, however, that evaluators may adjust from their initial assumptions through a far less effortful, nearly automatic impression of the alibi story and alibi provider, i.e., peripherally-processing the alibi. This kind of heuristic processing could conceivably lead evaluators to make judgments similar to those made under more effortful processing. For instance, the tone of voice of the detective as she makes her statements about the evidence may have been a simple cue to cognitively-busy evaluators that the alibi was or was not particularly strong. The nonverbal reactions of the alibi provider to the statements of the detective could also serve as a peripheral cue to cognitively-busy evaluators. The current experiment differs from many cognitive busyness experiments in that it used a stimulus with more potential peripheral,

simple cues than many experiments. For example, Gilbert et al. (1990) and Skurnik et al. (2003) used as stimulus materials phrases in which participants learned the definitions of nonsense words. While reading these phrases, participants were working with purely verbal, written information and thus had no peripheral information on which they might base a judgment. In the current experiment, however, participants were faced with a video of a person providing an alibi—giving them visual, nonverbal, and verbal information, some of it peripheral to the content of the alibi. In the persuasion literature, cognitively-busy message recipients are generally unable to centrally-process a message but can still process, and be influenced by, peripheral cues (see Petty, Wegener, & Fabrigar, 1997). I will refer to the idea that heuristic processing could lead alibi evaluators to make judgments similar to those made under more effortful processing as the superficial-evaluation hypothesis.

The pattern of correlations of the individual trait measures to the belief ratings offers some support for the superficial-evaluation hypothesis. The irrelevant traits should be irrelevant to the evaluation of an alibi if the evaluator is restricting his or her judgment to the central facts of the case. If, on the other hand, evaluators are forming more general impressions of the alibi provider from peripheral cues, I would expect a more generalized halo phenomenon (Nisbett & Wilson, 1977). In the case of a strong alibi, superficial evaluators would rate the alibi as more believable and also would have a positive overall impression of the alibi provider, rating him highly on positive traits and less highly on negative traits, even those irrelevant to the alibi. In the case of a weak alibi, superficial evaluators would rate the alibi as less believable and also would have a negative overall impression of the alibi provider, rating him highly on the negative traits and less highly on positive traits, even those irrelevant to the alibi. Thus I would expect the belief rating to be

more highly correlated with the irrelevant traits in cognitively-busy conditions (where presumably a superficial evaluation is taking place) than in non-cognitively-busy conditions. As seen in Table 4, this is indeed the case: The irrelevant traits were correlated with belief to a significantly greater degree in the busy conditions than in the non-busy conditions, $z = 2.59, p < .05$, one-tailed. This pattern does not hold for the detective belief or alibi strength measure, but these measures are qualitatively different than the belief measure—the belief measure asked participants about their own belief in the alibi, whereas the detective belief and strength measures asked participants for a more remote type of judgment.

The superficial-evaluation hypothesis at first appears inconsistent with participants' answers to the multiple-choice manipulation check questions—after all, nearly all cognitively-busy participants answered both questions correctly. However, the manipulation check questions do not definitively rule out the possibility that participants were relying on peripheral cues to make their evaluations. If participants had heuristically formed an impression of the alibi, they could easily infer from the two options in each question which option referred to a weak alibi and which option referred to a strong alibi. For example, the first manipulation check question asked, "When the detective said she talked to the man's sister, what was the sister's reaction?" Participants could answer "A: The sister was pretty sure she had called her brother for help that Saturday" or "B: The sister was quite certain her brother fixed her car that Saturday." Even without specifically recalling what the detective said during the video (due to being cognitively busy), a participant with an impression that the alibi was weak would choose answer A, which is in fact correct for the weak alibi video. A participant with an impression that the alibi was strong would choose answer B, the correct answer for the strong alibi video. The second manipulation check question asked, "What did

the detective say regarding the man's visit to the NAPA store?" Faced with the choices "A: The man's purchase was listed on his credit card records" or "B: The clerk at NAPA remembered the purchase and did not recognize the man's picture," a participant with an impression that the alibi was strong would likely choose answer A, the correct option for the strong alibi video. A participant with an impression that the alibi was weak would choose answer B, the correct option for the weak alibi video.

I believe the cognitive busyness manipulation worked to disrupt close scrutiny of the alibi by evaluators. Unanticipated in this experiment was the possibility that people can evaluate alibis (by making judgments about the strength of the alibi) based on simple cues—perhaps perceived nervousness of the alibi provider or possible skepticism in the tone of the interviewer. The only taxonomy to classify alibis according to their believability completely neglects the potential role of peripheral cues in alibi evaluation (Olson & Wells, 2004a). In their work, Olson and Wells assume the alibi evaluator operates almost completely on the content of the alibi and uses careful scrutiny to make an evaluation. The findings of the current experiment suggest possibilities for future research. For instance, the superficial-evaluation hypothesis suggests that the method used for studying alibi evaluation could be more important than first realized—asking evaluators to read an alibi statement would miss the potential peripheral cues that a videotaped alibi would provide, and perhaps an interaction situation with a participant as the interviewer would more closely capture the complexities of the alibi situation. However, the findings of the current experiment also mean this experiment cannot speak definitively about the starting point from which alibi evaluators begin their evaluations or to the idea that people are generally skeptical about alibis. Using a different methodological approach, perhaps Experiment 2 can speak to the alibi skepticism hypothesis.

EXPERIMENT 2: USING MEMORY TO INFER HOW EVALUATORS PROCESS ALIBIS

The starting point—be it an assumption of deception or an assumption of truth—from which the alibi evaluator begins his or her evaluation could conceivably have a substantial impact on the subsequent processing of the alibi. I propose that evaluators tend to organize an alibi story in a different fashion than they organize an average autobiographical account. First it would be useful to look at how people generally organize narrative information in memory and compare that to how alibi information might be organized differently.

Narrative information, such as a story told by a friend, is often organized in a schematic fashion. Schemas are generic memory structures that represent knowledge about a concept or stimulus (Alba & Hasher, 1983; Fiske & Taylor, 1991). Schemas may be thought of as knowledge structures used to organize and comprehend information—containing 'slots' for important elements and making those elements more likely to be learned (Anderson & Pichert, 1978). Scripts are a specialized type of schema: Fiske and Taylor (1991) defined a script as an event schema which may or may not delineate the sequence of a well-known situation. Although most Americans have not had experience providing an alibi in a legal situation, I would argue that people generally have a fairly detailed schema, perhaps even a script, for alibis. Fictional alibis are shown being given and evaluated in movies and on television programs; real-life alibis are discussed in newspaper and television news coverage of local and national criminal cases. An alibi schema might involve 'slots' for features of the alibi provider, the detective inquiring about the alibi, the time period the detective is interested in, and the alibi story that is provided. Activation of the alibi schema may also activate a number of related concepts, like deception and criminality. It is not unreasonable,

then, to hypothesize that evaluators would be suspicious of an alibi. According to Buller and Burgoon's (1996) interpersonal deception theory, suspicious people tend to exhibit a 'lie-bias;' that is, suspicious people tend to judge a storyteller as lying even when the storyteller is telling the truth. Burgoon, Buller, Ebesu, and Rockwell (1994) noted that suspicion alters an evaluator's attention and attributions, possibly introducing processing errors into an evaluator's memory for the interaction. Schul, Burnstein, and Bardi (1996) noted that suspicious people tend to encode information in ways that make it easier to discount the information when they find out it is untrue (see also Schul, 1993). Successful discounting is more likely when a person can generate a plausible counterscenario, or alternative explanation. As mentioned earlier, the "alibi provider is lying" explanation for an alibi is an easily generated alternative explanation and is likely to be salient to alibi evaluators.

According to schema theory as outlined by Alba and Hasher (1983), once a schema is activated, information is selectively encoded into memory according to its relevance to the active schema; more relevant information is encoded and remembered more easily than less relevant information. Anderson and Pichert (1978; see also Pichert & Anderson, 1977) asked participants to read a story about two boys playing in a house from either the perspective of a home buyer interested buying in the house or that of a burglar interested in burgling the house—that is, they induced one of two schemas in their participants. They found that participants recalled more information relevant to their perspective than information relevant to the opposite perspective. Wyer, Srull, Gordon, and Hartwick (1982) obtained similar results: participants who were given one of the two schemas before reading the story recalled more schema-relevant information and less schema-irrelevant information than those who did not have a schema prior to reading the story. This evidence suggests that to the extent that

participants have an active alibi schema, they will encode and recall information consistent with that schema. Thus, evaluators should pay special attention to information relating to the time of the crime, the places the alibi provider said he or she was, and the types of proof the alibi provider brings to support the alibi. Priester and Petty (1995) showed that people engage in deeper message scrutiny when the source of the message is of questionable honesty, and the nature of an alibi renders the source of that alibi a person of questionable honesty. Alibi schema effects and increased scrutiny of the alibi story would result in an evaluator more successfully encoding and recalling details central to the alibi and less successfully encoding and recalling details peripheral to the alibi.

The knowledge that the story is an alibi story could also arouse expectations about the alibi and the alibi provider, possibly influencing how evaluators encode and recall alibi information. Much of the past research on the effects of expectations on perception, belief, and memory has involved only expectations regarding the self (see Kunda, 1990, for a review). People tend to report that past attitudes and behaviors are more consistent with their current expectations, attitudes, and behavior patterns than they actually were. Ross and Conway (1986) proposed a model of autobiographical recall in which people tend to use their present standing on personal attributes as an anchor from which to work. People then reconstruct their past attitudes and behaviors using their present-oriented anchor and their intuitive theories about their likely change over time.

Hirt and colleagues have since examined the effect of people's expectancies on beliefs (Hirt, 1990) and memory (Hirt, McDonald, & Erickson, 1995; McDonald & Hirt, 1997) for another target person other than the self. Hirt (1990) argued in his general model of reconstructive memory that memory for other people works in the same fashion as memory

for the self—during retrieval (and thus reconstruction of the memory), people integrate information from three sources: the original memory trace, the present information, and an expectancy regarding possible change between the past and present. Much like an expectation about how the self has changed over time can bias processing and recall to influence reports of past behavior, an expectation about another person can bias processing and memory to influence reports about the other person's behavior. For instance, McDonald and Hirt (1997) gave participants information about a student's (Joe's) midterm scores in a class. They then produced an expectancy that Joe's grades would improve over time or decline over time. When given Joe's scores on the final exam, participants who expected improvement recalled that Joe had a lower midterm score than did participants who expected decline. Snyder and Uranowitz (1978) also presented evidence that expectations influence retrieval and reconstruction of information from memory. They presented participants with a case history of a woman's life. Sometime after reading the case history, participants learned the woman had been living either a lesbian or heterosexual lifestyle. This label influenced participants' memory about the events in the woman's life—when they committed memory errors, they erred in the direction of their new beliefs about the woman. Thus participants' expectations influenced the information recalled (Snyder & Uranowitz, 1978). In the case of the alibi evaluator, expectations may take the form of the assumption of disbelief—a skeptical anchor.

Anchoring effects occur when people begin with a starting point (often uninformative or extreme) and insufficiently adjust their judgment away from that point, resulting in judgments that are biased in the direction of their original starting point (Epley & Gilovich, 2002; Tversky & Kahneman, 1974). Chapman and Johnson (2002) argued that anchoring

effects occur because the anchor acts as a suggestion, a transient hypothesis that the evaluators entertain while formulating a judgment. This in turn increases the availability of anchor-consistent information in memory and decreases the availability of anchor-inconsistent information in memory. Thus if an alibi evaluator begins from an assumption of deception, they might recall more inconsistencies and suspicious tidbits from the alibi provider's story. Merely entertaining a hypothesis is often enough to increase one's confidence that the hypothesis is true (Koehler, 1991; Nickerson, 1998); alibi evaluators may also be more confident in their judgment that the alibi provider is deceptive than would observers who did not know it was an alibi.

The paradigm used by Anderson and Pichert (1978) and Wyer and colleagues (1982) to test schema effects on memory involved providing participants with one of two schemas (that of a homebuyer or that of a burglar) and testing how much information the participants remembered. I adapted this paradigm to investigate ways in which people process a story they believe is an alibi as opposed to a non-alibi story. Wyer and colleagues (1982) presented participants with one of two schemas (processing objectives) either before reading a story or after reading a story. Giving participants a schema before reading a story allows one to assess the effects the schema had on both encoding and retrieval of the story. Giving participants a schema after reading a story allows one to assess the effects of the schema only on retrieval of the story—presumably a schema might offer cues that would aid in the search for story information. In the current experiment, I manipulated the schema of some of the alibi evaluators (by giving them information about the alibi provider); whereas I gave other alibi evaluators no predetermined schema. By initiating the alibi schema at different points in their

processing of the alibi story, I hoped to discover something about how evaluators process alibis as compared to other narrative accounts.

Overview

In the current experiment, I manipulated whether participants viewed the video as an alibi or whether they viewed the video as a narrative account, only discovering later that the video story was an alibi. An active alibi schema could bias recall toward the period of time in the story most relevant to the alibi—the critical time period. For instance, evaluators with an active alibi schema could encode the information from the critical time period differently than those without such a schema, paying less attention to information outside the critical time period. Alternatively, evaluators could selectively retrieve more information from the critical time period than other time periods. A third possibility indicates recall could be biased at both encoding and retrieval. By comparing the pattern of recall from those whose alibi schema was activated prior to viewing the video with the pattern of recall from those whose alibi schema was activated after viewing the video, I could assess the effects an existing alibi schema has on encoding of the story. By comparing the pattern of recall from those whose alibi schema was activated after viewing the video with the pattern from those whose alibi schema was not activated until after completing the recall measure, I could assess the effects of an alibi schema on retrieval of the story. Finally, comparing the pattern of those whose alibi schema was active prior to viewing the video with the pattern from those whose alibi schema was active after completing the recall measure, I could asses the effects of the alibi schema on both encoding and retrieval of the alibi story. I hypothesized that of all information recalled, the proportion of information from the critical time period would be greatest in conditions where participants' alibi schema was activated before viewing the

video, due to effects of the schema on both encoding and retrieval. I would expect that selective retrieval alone can also bias recall toward the critical period, but perhaps this is a somewhat lesser effect.

The current experiment not only manipulated the timing of alibi schema activation but also manipulated what type of information participants received along with the alibi information. At the same time that participants were told the video was an alibi story, some participants were also told that the alibi provider had ultimately been found either guilty or innocent. Thus, some participants had directional alibi schemas, in which they knew the alibi provider was either lying (he was in fact guilty) or telling the truth (he was in fact innocent). It is less clear what to expect in the case of these directional alibi schemas. On the one hand, a knowledge of the actual fact of the alibi provider's innocence or guilt might nullify the critical-time-period recall bias—since there is no 'mystery' to the alibi, there would be no need for the evaluator to scrutinize the statements of the alibi provider relating to the critical time period. On the other hand, it is also possible that those who are told the alibi provider was guilty of the crime might continue to exhibit a critical-time-period bias because of their knowledge that the provider is lying—they would want to 'catch' him in the lie.

Lastly, the current experiment can also speak to some degree about the default assumptions of alibi evaluators—does an alibi schema distort processing in some way? By comparing the pattern of recall of those with an alibi schema with the pattern of those with a directional alibi schema, I could examine whether people without knowledge of the status of the alibi provider exhibit a pattern of recall more similar to those who know the provider is guilty, or whether they exhibit a similar pattern to those who know the provider is innocent.

Method

Participants. Participants were 322 undergraduate students from a large Midwestern university recruited for an experiment entitled "The Psychology of Belief." Participants earned extra credit in psychology classes for their participation. All participants were treated in accordance with the "Ethical Principles of Psychologists and Code of Conduct" (American Psychological Association, 2002). Due to a bug in the computer program used to collect data, recall data from 30 participants was not recorded. Seven other participants were excluded because they had previously participated in Experiment 1. These 37 participants were not included in the analysis, leaving a total of 285 participants.

Design. The design for Experiment 2 was a 2 x 3 +1 factorial design, illustrated in Figure 2. Participants were randomly assigned to one of seven conditions, which varied according to the type of information participants were given and the timing of that information.

	Type of Alibi Information		
Timing of Information	Alibi	Alibi *and* the provider is Innocent	Alibi *and* the provider is Guilty
Before viewing video			
After viewing video			
After primary measures			

Figure 2. Design of Experiment 2.

The type of alibi information participants received varied as follows: Participants in the *alibi-only* condition were told that the video story was an alibi. Participants in the *alibi-innocent* condition were told that the video story was an alibi and the alibi provider was ultimately proved innocent of the crime. Participants in the *alibi-guilty* condition were told that the video story was an alibi and the alibi provider was ultimately proved guilty of the crime.

The timing of the information varied as follows: Participants in the *before viewing video* conditions were told the alibi information before they saw the video, and participants in the *after* conditions were asked simply to watch a video of a person telling a story. Participants in the *after viewing video* condition were told the alibi information immediately following the video and prior to completing any measures. Participants in the *after primary measures* condition were told the alibi information after they had completed the recall and memory measures.

Materials. All participants viewed the same video of a 25-year-old Caucasian male providing an alibi. The video was identical to those used in Experiment 1, except the investigator did not interrupt the alibi provider to discuss the evidence supporting the alibi. See Appendix C for the script used for the videotaped alibi. The video was approximately four minutes long.

Procedure and Dependent Measures. All participants were asked to watch a video and form judgments about the person in the video, who was asked to talk about what he did the previous Saturday between 12:00 pm and 1:00 pm. Then participants were escorted into separate cubicles where they were seated at a computer to view the video of the alibi provider telling his story. At this time, participants in the *before viewing video* conditions were given

the alibi information. After the video ended, participants in the *after viewing video* conditions were given the alibi information. All participants were then given a 5-minute distractor task (completing mazes on the computer).

After participants completed the distractor task, they were asked to report as much of the alibi as they could remember (adapted from Wyer et al., 1982):

> A final thing we are interested in is how well people are able to recall information they receive. In the space below, please type as much of the original material from the video as exactly as you can. If you cannot remember the exact words used but you do remember its meaning, type the phrase as close to the original as possible. You will have 10 minutes to work. Please be sure to include every bit of the material you can remember, no matter how inconsequential it may seem.

As a manipulation check, 154 participants were randomly assigned to answer a group of 15 multiple-choice questions (hereafter referred to as the *multiple-choice memory test*) regarding what they remembered from the video (see Appendix D for the questions). Participants in the *after primary measures* condition were then given the alibi information, and all participants were then asked the same measures as in Experiment 1 regarding their belief in the alibi and their trait ratings of the alibi provider.

After participants completed all measures, they were fully debriefed, thanked, and dismissed.

Results

The Recall Measure: Scoring. To create a scoring scheme for the recall paragraphs, four independent raters parsed the video script into idea units, the smallest units of speech, or phrases, that convey a complete idea. Disagreements about specific phrases were discussed

and a consensus was reached. These phrases were then categorized into three blocks corresponding to one of the three time-frames of the story: 11:00-12:00, 12:00-1:00 (the critical time period, relevant to the alibi), and 1:00-2:00 (see Appendix C for phrase breaks and time block breaks). There were 19 phrases in the 11-12 time block, 28 phrases in the 12-1 time block, and 23 phrases in the 1-2 time block. Thus, phrases from the critical time period made up 40% of the total possible phrases to be recalled.

Then two independent scoring raters (separate from the parsing raters) scored each recall paragraph. Raters were kept blind to the condition to which participants had been assigned, and they used a scoring sheet with the phrases and time blocks the parsing raters had created. Raters scored the recall paragraphs as follows (see Appendix E for mean recall scores): If participants reported a phrase exactly as it was stated in the video, that phrase was given a score of 2. If participants reported the gist of the phrase as it was stated in the video (but did not report the phrase exactly), that phrase was given a score of 1. Each time block thus received two scores from each rater: a *perfect recall* score, the frequency with which phrases were scored a 2, and a *gist recall* score, the frequency with which phrases were scored a 1. Interrater reliability for the perfect recall score was 0.88, 0.84, and 0.87 for the 11-12, 12-1, and 1-2 time blocks, respectively. Reliability for the gist recall score was 0.69, 0.58, and 0.67 for the 11-12, 12-1, and 1-2 time blocks, respectively.

The gist recall score and the perfect recall score were then summed to yield the *total recall* score for each time block; thus, perfect recall was weighted more heavily in the total recall score than gist recall. Interrater reliability for the total recall score was 0.96, 0.94, and 0.94 for the 11-12, 12-1, and 1-2 time blocks, respectively. Because they showed the greatest interrater reliability, I used the total recall scores in the following analyses and averaged the

total recall score from both raters to achieve a composite total recall score for each recall paragraph. Summing the total recall scores from all time blocks yielded an *overall recall* score, which was used to calculate the *critical time bias score*, which was the proportion of overall recall that came from the 12-1 time block (the time period critical to the alibi). The critical time bias score was calculated by taking the total recall score from the 12-1 time block and dividing by the overall recall score (see Table 5).

Table 5. Critical time bias score (proportion of total information recalled from the critical time period) as a function of type and timing of alibi information

	Type of Alibi Information			
Timing of Information	Alibi	Alibi/Guilty	Alibi/Innocent	Total
Before viewing video	.47 (.13)	.43 (.11)	.40 (.09)	.43 (.11)
After viewing video	.42 (.09)	.46 (.13)	.44 (.10)	.44 (.11)
After primary measures	.42 (.10)			
Total	.44 (.11)	.45 (.12)	.42 (.10)	

Note: Standard deviations are given in parentheses.

The Recall Measure: Analysis. There was no theoretical reason to believe that overall recall would differ among the conditions of the experiment, and indeed participants across conditions did not differ in the overall amount of information they recalled $F(6, 278) = 0.76$, $MSE = 299.79$, $p > .05$. The hypotheses for this experiment concerned the relative amount of recall from the critical time period compared to the non-critical time periods; thus the primary analyses were conducted on the critical time bias score. Did the timing and type of

alibi information make a difference in the amount of information recalled from the critical time period? An overall oneway ANOVA revealed a significant effect $F(6, 277) = 2.60$, $MSE = 0.01$, $p < .05$. Thus, a series of planned comparisons were then conducted to investigate the hypotheses (see Table 6). Contrasts 1 and 2 tested the effects of the timing of an alibi schema on recall of the alibi story. Did knowledge that the story was an alibi bias evaluators' recall toward alibi-relevant information? When participants knew the story was an alibi prior to viewing the video (the *alibi-only, before viewing video* condition), they exhibited a significantly greater critical time period bias than participants who did not know the story was an alibi (the *alibi-only, after primary measures* condition) $t(277) = 2.30$, $p < .05$, $d = 0.28$ (Contrast 1). However, when participants discovered the story was an alibi after viewing the video (the *alibi-only, after viewing video* condition), they did not exhibit a greater critical time period bias than participants who did not know the story was an alibi (the *alibi-only, after primary measures* condition) $t(277) = 0.03$, $p > .05$, $d = 0.003$ (Contrast 2). Thus, an alibi schema appears to influence encoding, but not retrieval, of alibi information.

Did the knowledge that the alibi provider was guilty have the same effect when the information came before the alibi story as it did when it came after the alibi story? Contrast 3 examined the interaction between the guilty alibi schema and alibi-only schema with the timing of the schema activation (before or after the alibi video), and this interaction was significant $t(277) = 2.72$, $p < .05$ $d = 0.33$. Because the interaction was significant, three additional contrasts were conducted to test the simple effects. In the *before viewing video* conditions, participants in the *alibi-guilty* condition did not exhibit greater critical time period bias compared to those in the *alibi-only* condition $t(277) = 1.89$, $p > .05$, $d = 0.23$ (Contrast 4). Similarly, in the *after viewing video* conditions, participants in the *alibi-guilty*

Table 6. Planned single-degree-of-freedom contrasts on critical time bias score

	Comparison								
Condition	1	2	3	4	5	6	7	8	9
Alibi-only, Before video	1		.5	1		1	.5	1	
Alibi-guilty, Before video			-.5	-1					
Alibi-innocent, Before video							-.5	-1	
Alibi-only, After video		1	-.5		1	-1	-.5		1
Alibi-guilty, After video			.5		-1				
Alibi-innocent, After video							.5		-1
Alibi-only, After measures	-1	-1							
Significant at $p < .05$:	*		*			*	*	*	
Value of contrast:	0.06	0.00	0.05	0.05	-0.05	0.06	0.05	0.08	-0.02
SE:	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Cohen's *d*:	0.28	0.003	0.33	0.23	0.24	0.28	0.35	0.38	0.11

condition did not exhibit greater critical time period bias compared to those in the *alibi-only* condition $t(277) = 1.96, p > .05, d = 0.24$ (Contrast 5). However, participants in the *alibi-only, before viewing video* condition exhibited a significantly greater critical time period bias than those in the *alibi-only, after viewing video* condition $t(277) = 2.32, p < .05, d = 0.28$ (Contrast 6). This pattern of simple effects indicates that knowing that they were viewing an alibi before viewing the video increased biased recall if participants did not know that the alibi provider was guilty, but knowing the alibi provider was guilty nullified this effect.

Did the knowledge that the alibi provider was innocent have the same effect when the information came before the alibi story as it did when it came after the alibi story? Contrast 7 examined the interaction between the innocent alibi schema and the alibi-only schema with the timing of the alibi information, and this interaction was significant $t(277) = 2.88, p < .05, d = 0.35$. Thus, simple effects contrasts were conducted. In the *before viewing video* conditions, participants in the *alibi-only* condition exhibited a significantly greater critical time period bias compared to those in the *alibi-innocent* condition $t(277) = 3.19, p < .05, d = 0.38$ (Contrast 8), indicating that participants who knew before viewing the video that the alibi provider was truthful exhibited less of a critical time bias than participants who only knew the story was an alibi. In the *after viewing video* conditions, participants in the *alibi-only* condition did not exhibit greater critical time period bias compared to those in the *alibi-innocent* condition $t(277) = 0.88, p > .05, d = 0.11$ (Contrast 9).

Multiple-Choice Memory Test. A subset of 154 participants were randomly assigned to answer 15 multiple-choice questions regarding what they remembered from the video story. Participants were given 1 point for each question they answered correctly on the test, and participants' score on the memory test was the sum of points earned (see Table 7 for

mean scores). Participants did not differ according to condition in how much overall information they correctly remembered $F(6, 143) = 0.40, MSE = 2.86, p > .05$. Five of the multiple-choice questions concerned information from the critical time period, and participants did not differ according to condition in their scores for those five questions $F(6, 143) = 0.48, MSE = 0.85, p > .05$. Ten of the multiple-choice questions concerned information from the non-critical time periods, and participants did not differ according to condition in their scores for those ten questions $F(6, 143) = 0.56, MSE = 1.47, p > .05$.

Table 7. Mean scores on multiple-choice memory test

	Type of Alibi Information			
Timing of Information	Alibi	Alibi/Guilty	Alibi/Innocent	Total
Before viewing video	12.9 (2.2)	12.9 (1.7)	13.0 (1.3)	12.9 (1.8)
After viewing video	13.0 (1.6)	12.6 (1.5)	13.2 (1.5)	13.0 (1.5)
After primary measures	13.3 (1.8)			
Total	13.1 (1.9)	12.8 (1.6)	13.1 (1.4)	

Note: Standard deviations are given in parentheses.

The Belief Measures. Although not intended as a primary measure for Experiment 2, participants answered the same set of belief measures as in Experiment 1 (see Table 8 for means and standard deviations). Because of the intuitive absurdity of informing evaluators of a suspect's guilt or innocence prior to asking evaluators how much they believe an alibi, the belief measures from the *alibi-innocent* and *alibi-guilty* conditions were not considered theoretically interesting and were thus not included in the analysis. More useful and

theoretically interesting were the three conditions in which participants were not told the ultimate status of the alibi provider. The time at which participants were told the story was an alibi did not affect participants' belief judgments $F(2, 117) = 0.89$, $MSE = 5.11$, $p > .05$. Likewise, timing of the alibi schema did not affect participants' ratings of how believable a detective would find the alibi $F(2, 117) = 1.54$, $MSE = 5.47$, $p > .05$. Lastly, the time at which participants were told the story was an alibi did not affect participants' judgments of the strength of the alibi $F(2, 117) = 1.20$, $MSE = 2.47$, $p > .05$.

Table 8. Belief measures as a function of timing and type of alibi information

Belief

	Type of Alibi Information			
Timing of Information	Alibi	Alibi/Guilty	Alibi/Innocent	Total
Before viewing video	6.05 (2.17)	5.49 (2.55)	6.34 (2.69)	5.96 (2.47)
After viewing video	6.19 (2.38)	4.70 (1.93)	6.74 (2.10)	5.88 (2.14)
After primary measures	6.70 (2.22)			
Total	6.31 (2.26)	5.10 (2.24)	6.54 (2.40)	

Detective Belief

Timing of Information	Alibi	Alibi/Guilty	Alibi/Innocent	Total
Before viewing video	4.85 (2.07)	4.59 (2.23)	5.78 (2.60)	5.07 (2.30)
After viewing video	5.10 (2.42)	3.63 (1.62)	6.19 (2.34)	4.97 (2.13)
After primary measures	5.76 (2.52)			
Total	5.24 (2.34)	4.11 (1.93)	5.99 (2.47)	

Alibi Strength Rating

Timing of Information	Alibi	Alibi/Guilty	Alibi/Innocent	Total
Before viewing video	3.95 (1.41)	3.64 (1.55)	4.20 (1.44)	3.93 (1.47)
After viewing video	3.64 (1.69)	3.12 (1.52)	4.10 (1.54)	3.62 (1.58)
After primary measures	4.19 (1.60)			
Total	3.93 (1.57)	3.38 (1.54)	4.15 (1.49)	

Note: Standard deviations are given in parentheses.

The Trait Ratings. Although not intended as a primary measure for Experiment 2, participants also answered the trait rating questions from Experiment 1. As in Experiment 1, traits were divided into the two categories, relevant traits and irrelevant traits, used by Olson and Wells (2004a). Positive traits (*honest, open, sincere, trustworthy, loyal, intelligent, content, caring, curious, friendly, ambitious, funny*) were reverse-coded so that all scores carried a negative connotation (i.e., *trustworthy* became *untrustworthy*; see Appendix F for correlations among individual trait ratings and belief measures). Traits were then averaged to yield composite scores for both relevant and irrelevant traits (see Table 9 for means and standard deviations).

Table 9. Mean composite trait ratings as a function of timing and type of alibi information

Relevant Traits

	Type of Alibi Information			
Timing of Information	Alibi	Alibi/Guilty	Alibi/Innocent	Total
Before viewing video	4.22 (0.67)	4.12 (0.66)	3.79 (0.76)	4.04 (0.72)
After viewing video	3.93 (0.75)	4.32 (0.76)	3.77 (0.75)	4.01 (0.79)
After primary measures	4.02 (0.85)			
Total	4.06 (0.76)	4.23 (0.72)	3.78 (0.75)	

Irrelevant Traits

Timing of Information	Alibi	Alibi/Guilty	Alibi/Innocent	Total
Before viewing video	4.22 (0.68)	4.29 (0.69)	3.98 (0.68)	4.16 (0.69)
After viewing video	4.45 (0.66)	4.54 (0.65)	4.35 (0.62)	4.45 (0.64)
After primary measures	4.26 (0.73)			
Total	4.31 (0.69)	4.42 (0.68)	4.17 (0.67)	

Note: Standard deviations are given in parentheses. All traits coded to have a negative connotation; higher composite scores mean a more negative evaluation.

Did the timing and type of alibi information make a difference in participants' relevant trait ratings of the alibi provider? An overall oneway ANOVA revealed a significant effect $F(6, 278) = 3.32, MSE = 0.56, p < .05$. Thus several contrasts were conducted to determine the nature of the differences. Did the type of alibi information affect participant's relevant trait ratings? A oneway ANOVA comparison showed a significant effect $F(2, 282) = 7.58, MSE = 0.56, p < .05$. Individual contrasts revealed no significant difference in relevant trait ratings from participants with the guilty alibi schema (the *alibi-guilty* conditions) and participants with the nondirectional alibi schema (the *alibi-only* conditions) $t(282) = 1.58, SE = 0.11, p > .05$. However, there was a significant difference in relevant trait ratings from participants with the innocent alibi schema (the *alibi-innocent* conditions) and participants with the non-directional alibi schema $t(282) = 2.60, SE = 0.11, p < .05$; there was also a significant difference in ratings from participants with the guilty alibi schema and participants with the innocent alibi schema $t(282) = 3.84, SE = 0.12, p < .05$. Participants with an alibi schema who did not know the ultimate guilt or innocence of the alibi provider thus provided relevant trait ratings more similar to those who knew the alibi provider was guilty rather than those who knew the alibi provider was innocent. Did the timing of alibi information affect participants' relevant trait ratings? A oneway ANOVA comparison showed that it did not $F(2, 282) = 0.06, MSE = 0.59, p > .05$.

Did the timing and type of alibi information make a difference in participants' irrelevant trait ratings of the alibi provider? An overall oneway ANOVA revealed a significant effect $F(6, 278) = 2.93, MSE = 0.49, p < .05$. Thus several contrasts were conducted to determine the nature of the differences. Did the type of alibi information affect participant's irrelevant trait ratings? A oneway ANOVA comparison showed that it did not

$F(2, 282) = 2.77, MSE = 0.47, p > .05$. Did the timing of alibi information affect participants' irrelevant trait ratings? A oneway ANOVA comparison revealed a significant effect $F(2, 282) = 5.63, MSE = 0.46, p < .05$. Individual contrasts revealed no significant difference in irrelevant trait ratings from participants who learned the story was an alibi before the video compared to participants who learned the story was an alibi after the primary measures $t(282) = 0.75, SE = 0.13, p > .05$. Likewise, there was no significant difference in irrelevant trait ratings from participants who learned the story was an alibi after the video compared to participants who learned the story was an alibi after the primary measures $t(282) = 1.51, SE = 0.13, p > .05$. However, participants who learned the story was an alibi before the video rated the alibi provider significantly less negatively on the irrelevant traits than did participants who learned the story was an alibi after the video $t(282) = 3.33, SE = 0.09, p < .05$.

Discussion

An active alibi schema could alter how people process a narrative account by causing them to engage in increased processing of information most relevant to an alibi at the expense of processing less relevant details. Thus, the recall of a narrative account by someone who had an active alibi schema would be more heavily concentrated on the time markers in the story and the information contained within the critical (i.e., alibi-relevant) time period compared to someone without an active alibi schema. This effect could arise one of three ways: through selective encoding, selective retrieval, or both.

There was no evidence of a reliable bias in processing at the level of retrieval. By comparing the *alibi-only, after viewing video* condition with the *alibi-only, after primary measures* condition, I could assess the affects of an alibi schema on retrieval of the alibi story. Participants who discovered after the video that they had seen an alibi had 42% of their

overall recall come from the critical time period, which was not different from the amount of information from the critical time period recalled by participants who completed the recall measure without knowing that the story was an alibi. Also, the *alibi-guilty, after viewing video* condition (at 46%) and the *alibi-innocent, after viewing video* condition (at 44%) were not different from the *alibi-only, after video* condition (at 42%), indicating that a directional alibi schema does not bias retrieval.

There was, however, evidence of a bias in processing toward alibi-relevant information at the level of encoding. The difference between the *alibi-only, before viewing video* condition and the *alibi-only, after viewing video* condition is the effect of the alibi schema on encoding of the information from the video. Participants who knew before watching the video that they were watching an alibi had a greater proportion of their total recall come from the critical time period (47%) than did participants who did not know they saw an alibi until after the video (42%). Consider that critical-time-bias score from the *alibi-only, after primary measures* condition represents the base-rate score, as participants in this condition viewed the video and answered the primary measures under the impression that the story was simply a narrative account. Participants who knew prior to the video that they were seeing an alibi also had a greater critical time bias than the base rate (47% to 42%, respectively). Interestingly, knowledge that the alibi provider was innocent prior to viewing the video nullified the tendency to bias recall toward the critical time period (40% compared to 47% in the *alibi-only* condition).

The results of Experiment 2 indicate that alibis elicit different processing from a recipient of a story than do everyday memory statements. One way alibis differ from everyday memory statements is that alibis must solve a time/space problem: The alibi must

'prove' that at the time of the crime, the alibi provider was somewhere else from the scene of the crime (Olson, 2002). An alibi evaluator will likely be tuned to information relating to the time/space problem and focus his or her attention and processing resources to that information. The current research suggests that this additional processing of alibi-relevant, time/space information is largely the result of processing at encoding (be it selective encoding or simply increased processing).

Although retrieval effects did not achieve statistical significance in the current study, it is possible that a critical-time bias might surface during retrieval under different circumstances. One possibility is simply that the effect of an alibi schema on selective retrieval is weaker than the effect of an alibi schema on encoding, and that with sufficient power, a retrieval effect would become evident. However, a more compelling argument for why retrieval effects may not have surfaced in the current experiment is due to the peculiar nature of an alibi compared to the everyday memory statement. For an everyday memory statement, the kinds of stories that people often share, details about time and location are less important than for an alibi statement. When a receiver does not know that the story is an alibi, he or she does not know what times are relevant to the alibi, and perhaps does not encode *any* time markers. Then, when the receiver discovers the story had been an alibi, the receiver does not have the time markers necessary to direct retrieval to the relevant time frame. Thus, no matter how motivated an evaluator might be at that point to selectively recall only information from the critical time period, the evaluator would be unable to do so.

In the current experiment, the finding of a critical time bias (a recall bias toward alibi-relevant information) is especially interesting because the alibi-relevant information in this particular operationalization of an alibi appeared during the middle part of the

videotaped story and thus had to overcome both the primacy and recency effects to be represented to a greater degree in recall (Atkinson & Shiffrin, 1971).

While not primary measures in Experiment 2, the belief measures and trait ratings provide interesting fodder for consideration. Participants' belief in the alibi provider's story did not change as a function of the amount of time the participants operated under the belief that the story was an alibi. Interestingly, relevant trait ratings from participants who did not know the ultimate status of the alibi provider ($M = 4.06$) were more similar to the relevant trait ratings from participants who believed the alibi provider was guilty ($M = 4.23$) than the ratings from participants who believed the alibi provider was innocent ($M = 3.78$). This suggests some evidence in support of the alibi skepticism hypothesis—an evaluator uncertain of the truth status of the alibi provider might interpret nonverbal cues from the alibi provider as indicative of deceit. This is particularly likely with police detectives, who might have a base-rate expectation that a given alibi provider will be lying to them (Eckman, 2001).

GENERAL DISCUSSION

One of the original purposes of this research was an empirical test of the alibi skepticism hypothesis: that people are skeptical and disbelieving when evaluating alibi information compared to other types of information. Unfortunately the findings of the two studies in this dissertation do not present overwhelming evidence in support of the alibi skepticism hypothesis.

Experiment 1 replicated Olson and Wells (2004a) insofar as it presented two alibis with different levels of evidence as supporting proof, and the strong alibi was considered more believable and was rated as stronger than the weak alibi. However, Experiment 1 was unable to speak to the starting point from which alibi evaluators begin. An underlying assumption of much of the alibi work to date is that evaluation of an alibi requires close scrutiny of the alibi information (Burke & Turtle, 2003; Culhane & Hosch, in press; Olson & Wells, 2004a). It was critical that the cognitive busyness manipulation interrupt the central, controlled processing of the alibi information so that the starting point from which evaluators begin might influence the belief ratings of the evaluators. Unfortunately, the cognitive busyness manipulation did not significantly affect participants' belief ratings, which were the primary measures of the experiment. Previous research has shown that when there is an a-priori bias to believe or disbelieve, cognitive busyness manipulations can successfully reveal the starting point or bias (Gilbert et al., 1993; Skurnik et al., 2003). The failure of the busyness manipulation to interact with alibi strength is not consistent with the alibi skepticism hypothesis—that people begin with a propensity to disbelieve the alibi. But, as discussed earlier, it is also not consistent with the hypothesis that people begin with a propensity to believe. Nor are the data consistent with the hypothesis that people begin with a

neutral starting point. All three hypotheses predict an interaction between cognitive busyness and alibi strength. A fourth possibility is that some people begin with a starting point of disbelief (perhaps those who are naturally skeptics) while other people begin with a starting point of belief (the naturally trusting). Through random assignment, these two subpopulations are likely to be represented equally in all groups, in which case they would counteract each other, leading to a prediction of an interaction similar to that of a neutral starting point. Thus, the data are not consistent with the possibility of differing starting points, either.

While it is possible that the cognitive busyness manipulation did not tax participants adequately, a possibility previously unforeseen in this work is that central processing is not required for evaluation of an alibi to take place—that peripheral, shallow processing can produce similar judgments of belief and alibi strength as more central, deep processing. This superficial evaluation hypothesis seems consistent with a pattern of results that emerged in the irrelevant trait ratings. Irrelevant trait ratings and participants' belief were more highly correlated in the cognitively-busy conditions than in the non-cognitively-busy conditions, suggesting a more pronounced halo effect in the ratings of the alibi provider from cognitively-busy participants. This is precisely the kind of effect one might expect if participants are basing their judgments regarding the believability of the alibi on a general impression formed from simple, peripheral cues, i.e. engaging in superficial evaluation of the alibi.

I intend to continue investigation into the superficial-evaluation hypothesis to determine the extent to which alibi evaluators use peripheral cues in their evaluation process. For example, would evaluators reach similar judgments as participants in the current study if

they were unable to view the video, and only had aural peripheral cues to rely upon? Would evaluators make similar judgments if they were unable to hear the alibi provider, and only had visual peripheral cues to rely upon? One way to prevent peripheral cues from influencing judgments would be to create video of an alibi provider giving an alibi and manually edit the video to include the information from the detective about the evidence supporting the alibi. In these new videos, the alibi provider would be reacting to the information from the detective in the same way for both a strong and a weak alibi, leaving the evaluator unable to determine the strength of the alibi from nonverbal peripheral cues from the alibi provider. Would cognitively-busy evaluators still produce similar judgments as non-cognitively busy evaluators? If the superficial-evaluation hypothesis is valid, cognitively-busy participants should not produce judgments similar to non-cognitively-busy participants when viewing videos that control nonverbal peripheral cues, and the starting point from which they begin their judgment should surface in their judgment.

Experiment 2 was designed to address the nature of evaluators' processing of alibis compared to processing other narrative accounts. As expected, when participants knew ahead of time that they were watching an alibi, their recall was biased toward the critical time period in which the crime occurred—a greater proportion of their total recalled information was information from the time period relevant to the alibi. However, when participants learned after viewing that they had watched an alibi, this recall bias did not occur. This suggests that processing differences arise during encoding, not at the time of retrieval. Also, knowledge that the alibi provider was innocent or guilty reduced the critical time period bias; perhaps participants who knew the ultimate status of the alibi provider felt less compelled to focus on critical time details because the case was 'solved.' Knowledge that the alibi

provider was innocent prior to viewing the video eliminated the critical time period bias, indicating that these participants treated the innocent person's alibi as if it were simply an autobiographical story and information from the critical time period appeared in their recall in the same proportion it appeared in the video.

Despite alternative explanations for the failure of the busyness manipulation in Experiment 1 to reveal a skepticism bias, serious consideration must be given to the possibility that the alibi skepticism hypothesis is incorrect. Perhaps the evaluator skepticism that surfaced in Olson and Wells (2004a) arose in part because of the barrenness and purely verbal nature of the manipulation—participants were given short paragraphs describing each alibi. People may tend to be less skeptical in the face of an alibi provider (like the videotaped alibi in the current study), similar to how people in deception experiments tend to judge people as generally truthful (O'Sullivan, 2003). Also, in the Olson and Wells study, participants were asked to take on the role of a detective, which may have served to make them more skeptical than they otherwise would have been. Note that in both the current experiments, participants' ratings of a hypothetical detective's belief in the alibis were always lower than their ratings of their own belief in the alibis. Although Experiment 2 was not designed specifically to examine the alibi skepticism hypothesis, aspects of the belief and trait data could be interpreted as being inconsistent with the skepticism hypothesis. Specifically, the belief ratings in the alibi-only conditions ($M = 6.31$, when participants did not know the ultimate status of the alibi provider) are much more similar to the alibi-innocent conditions ($M = 6.54$) than they are to the alibi-guilty conditions ($M = 5.10$). The alibi-only conditions were not significantly different from the alibi-innocent conditions $t(282) = 0.74$, $p > .05$, $d = 0.09$, but both the alibi-only and alibi-innocent conditions were significantly

different from the alibi-guilty conditions $ts(282) = 4.09$ and 3.72, $ps < .05$, $ds = 0.49$ and 0.44, respectively. On the other hand, the ratings of the alibi provider on traits relevant to an alibi (e.g., trustworthiness, sincerity, honesty) were consistent with the skepticism hypothesis: Participants who did not know the ultimate status of the alibi provider rated him similarly to those who knew he was guilty: more negatively than the ratings from participants who knew the alibi provider was innocent. However, these trait data are rather weak support for the skepticism hypothesis—if the alibi skepticism hypothesis was valid, evidence for it should have surfaced in the belief data. The trait ratings are more indirect and not as closely related to the skepticism idea as belief. On balance, the data are more consistent with the idea that evaluators approach alibis with belief rather than with skepticism; perhaps Spinoza was right after all.

That evaluators would be skeptical of an alibi story is an intuitively satisfying hypothesis. However, data from the current studies suggests that the alibi skepticism hypothesis may not be as robust as first proposed by this dissertation and Olson and Wells (2004a). Perhaps evaluators assume Grice's (1975) maxim of quality—that people are generally speaking truth—even when evaluating an alibi. Perhaps people want to believe the best about others. The skepticism hypothesis may be more evident in particular populations—people who have a high internal base-rate of deceptive behavior (natural skeptics) or police detectives, who may have considerable experience with fabricated alibis.

While the alibi skepticism hypothesis did not receive a great deal of support, the current research did provide some evidence that evaluators process alibis in a different fashion than they would process everyday memory statements. More research into the differences between alibis and everyday memory statements will clarify the nature of the

alibi schema and the processing it elicits. Lastly, the current research provides rich fodder for future study in the superficial-evaluation hypothesis—does evaluation require consideration for the central facts of a case, or can evaluators reach similar decisions using simple peripheral cues?

APPENDIX A

Weak Alibi Script

Investigator: So where were you, and what were you doing, between noon and one last Saturday?

Alibi Provider: Well, let's see...last Saturday, oh yeah, that was a hell of a day...I was planning on getting up to get some stuff done at the library, but my alarm clock didn't go off, and I woke up late, like 11:00. So I had some cheesy 80s movie on the TV (maybe *Howard the Duck*?) as I raced around trying to get my stuff together...I couldn't find anything, my watch was gone, my roommate nowhere to be found, my books were all over the floor, the keys were somewhere in the couch, you know how mornings like those go. I was scarfing down breakfast when my sister called and of course I had to find the cell phone...so she called about twenty after 11 and wanted me to come fix her car. Well, my sister lives all the way across town, so I was like, sure, I'll come check out your car, but I can't guarantee anything.

So I get to my sister's at quarter to noon and her car isn't messed up—it just needs some new belts and a battery. Trust my sis to be clueless.

Investigator: Yes, well, I talked to your sister, and she said she's *pretty* sure she called you for help on Saturday.

Alibi Provider: Well, I was there! Anyway, she's on her way out the door to work, and she got her boyfriend to drive her; he was just sitting in his car and honking the horn. So she leaves me the keys to the apartment and her car and tells me to fix it before she gets back home. She was all, "I'll pay you on Monday when I get paid" and I still haven't seen any of what she owes me. Anyway, I went over to NAPA to get the stuff I'd need for sis's car.

Investigator: The clerk at NAPA remembers somebody coming in to get belts and a battery, but he didn't recognize your picture...

Alibi Provider: I thought my friend Jeff was working that day, so I wandered all over the store looking for him. He usually works on Saturdays, but he must have been out to lunch or something, cuz I didn't see him there. I got the stuff I needed—I noticed as I grabbed my wallet my card was expired so I had to use most of my cash to pay for it

all. I stopped at Arby's on the way back to my sister's, cuz it was like 12:30 and I was hungry. And then there was this huge line and they gave me a plain roast beef when what I really ordered was a cheddar melt but I didn't discover that until I got back to my sister's.

I got the car fixed and finally headed to campus by 1:00. My sister has this little fake rock in the garden to leave the spare set of keys in; naturally I remembered about that when I was halfway down the block, so I had to cut a U-turn and put her keys away. Then I drove to campus. Of course the lot was full and I had to drive around to like 3 different lots before I found one I could park in. Just as I was gathering my stuff from my car I thought I remembered I had group meeting in the computer lab, so I ran for it. But then when I got there nobody was there, so I dug out my notebook and discovered the group meeting was the next day, Sunday, so I really wasn't missing anything! That was a real relief. So I hung out in the lab and surfed the web to check out flights for spring break. Two of my friends are headed to Mexico, so I wanted to see how much money that would cost me to join them. I was at the lab until probably 2:30, when my friend Ryan called my cell and told me that he was standing by my car and it had a ticket on it—I guess I forgot to pay the meter. Again.

APPENDIX B

Strong Alibi Script

Investigator: So where were you, and what were you doing, between noon and one last Saturday?

Alibi Provider: Well, let's see...last Saturday, oh yeah, that was a hell of a day...I was planning on getting up to get some stuff done at the library, but my alarm clock didn't go off, and I woke up late, like 11:00. So I had some cheesy 80s movie on the TV (maybe *Howard the Duck*?) as I raced around trying to get my stuff together...I couldn't find anything, my watch was gone, my roommate nowhere to be found, my books were all over the floor, the keys were somewhere in the couch, you know how mornings like those go. I was scarfing down breakfast when my sister called and of course I had to find the cell phone...so she called about twenty after 11 and wanted me to come fix her car. Well, my sister lives all the way across town, so I was like, sure, I'll come check out your car, but I can't guarantee anything.

So I get to my sister's at quarter to noon and her car isn't messed up—it just needs some new belts and a battery. Trust my sis to be clueless.

Investigator: Yes, well, I talked to your sister, and she was quite certain you fixed her car that Saturday.

Alibi Provider: Well, I was there! Anyway, she's on her way out the door to work, and she got her boyfriend to drive her; he was just sitting in his car and honking the horn. So she leaves me the keys to the apartment and her car and tells me to fix it before she gets back home. She was all, "I'll pay you on Monday when I get paid" and I still haven't seen any of what she owes me. Anyway, I went over to NAPA to get the stuff I'd need for sis's car, and I thought my friend Jeff was working that day, so I wandered all over the store looking for him. He usually works on Saturdays, but he must have been out to lunch or something, cuz I didn't see him there. I got the stuff I needed—I used my credit card so I'd have a record for my sister.

Investigator: Yes, I have here a copy of your credit card statement, and I see your purchase is on here...

Alibi Provider: I stopped at Arby's on the way back to my sister's, cuz it was like 12:30 and I

was hungry. And then there was this huge line and they gave me a plain roast beef when what I really ordered was a cheddar melt but I didn't discover that until I got back to my sister's.

I got the car fixed and finally headed to campus by 1:00. My sister has this little fake rock in the garden to leave the spare set of keys in; naturally I remembered about that when I was halfway down the block, so I had to cut a U-turn and put her keys away. Then I drove to campus. Of course the lot was full and I had to drive around to like 3 different lots before I found one I could park in. Just as I was gathering my stuff from my car I thought I remembered I had group meeting in the computer lab, so I ran for it. But then when I got there nobody was there, so I dug out my notebook and discovered the group meeting was the next day, Sunday, so I really wasn't missing anything! That was a real relief. So I hung out in the lab and surfed the web to check out flights for spring break. Two of my friends are headed to Mexico, so I wanted to see how much money that would cost me to join them. I was at the lab until probably 2:30, when my friend Ryan called my cell and told me that he was standing by my car and it had a ticket on it—I guess I forgot to pay the meter. Again.

APPENDIX C

Alibi Script—Experiment 2

Investigator: So where were you, and what were you doing, between noon and one last Saturday?

Alibi Provider: Well, let's see...last Saturday, oh yeah, that was a hell of a day/...I was planning on getting up to get some stuff done at the library,/ but my alarm clock didn't go off,/ and I woke up late, like 11:00./ So I had some cheesy 80s movie on the TV (maybe *Howard the Duck*?)/ as I raced around trying to get my stuff together/...I couldn't find anything,/ my watch was gone,/ my roommate nowhere to be found,/ my books were all over the floor,/ the keys were somewhere in the couch,/ you know how mornings like those go./ I was scarfing down breakfast/ when my sister called/ and of course I had to find the cell phone/...so she called about twenty after 11/ and wanted me to come fix her car./ Well, my sister lives all the way across town,/ so I was like, sure, I'll come check out your car,/ but I can't guarantee anything./ [End 1st time block]

So I get to my sister's at quarter to noon/ and her car isn't messed up/—it just needs some new belts and a battery./ Trust my sis to be clueless./ Anyway, she's on her way out the door to work,/ and she got her boyfriend to drive her;/ he was just sitting in his car and honking the horn./ So she leaves me the keys to the apartment and her car/ and tells me to fix it before she gets back home./ She was all, "I'll pay you on Monday when I get paid"/ and I still haven't seen any of what she owes me./ Anyway, I went over to NAPA to get the stuff I'd need for sis's car./ I thought my friend Jeff was working that day,/ so I wandered all over the store looking for him./ He usually works on Saturdays,/ but he must have been out to lunch or something,/ cuz I didn't see him there./ I got the stuff I needed/—I noticed as I grabbed my wallet my card was expired/ so I had to use most of my cash to pay for it all./ I stopped at Arby's on the way back to my sister's,/ cuz it was like 12:30/ and I was hungry./ And then there was this huge line/ and they gave me a plain roast beef when what I really ordered was a cheddar melt/ but I didn't discover that until I got back to my sister's./

I got the car fixed/ and finally headed to campus by 1:00./ [End 2nd time

block] My sister has this little fake rock in the garden to leave the spare set of keys in;/ naturally I remembered about that when I was halfway down the block,/ so I had to cut a U-turn and put her keys away/. Then I drove to campus./ Of course the lot was full/ and I had to drive around to like 3 different lots before I found one I could park in./ Just as I was gathering my stuff from my car/ I thought I remembered I had group meeting in the computer lab,/ so I ran for it./ But then when I got there nobody was there,/ so I dug out my notebook/ and discovered the group meeting was the next day, Sunday,/ so I really wasn't missing anything!/ That was a real relief./ So I hung out in the lab/ and surfed the web to check out flights for spring break./ Two of my friends are headed to Mexico,/ so I wanted to see how much money that would cost me to join them./ I was at the lab until probably 2:30,/ when my friend Ryan called my cell/ and told me that he was standing by my car/ and it had a ticket on it/—I guess I forgot to pay the meter. Again./ [End 3rd time block]

APPENDIX D

Memory Measures, Experiment 2 (Correct Answers in Bold)

1. The man in the video said he was planning to get up and get things done at:
   - A. **the library**
   - B. the bank
   - C. the laundromat
2. Where did the man say his sister lived?
   - A. **across town**
   - B. next door
   - C. another city
3. He said his keys were:
   - A. nowhere to be found
   - B. in his car
   - C. **somewhere in the couch**
4. His sister called to ask him:
   - A. to give her a ride to work
   - B. **to fix her car**
   - C. to lend her money
5. He arrived at his sister's place at approximately:
   - A. 10:00 AM
   - B. 10:45 AM
   - C. 11:15 AM
   - D. **11:45 AM**
6. Why did the man say he went to NAPA?
   - A. **to get parts for his sister's car**
   - B. to visit a friend
   - C. to pick up a check
7. The man said he wandered all over the store looking for someone. Who was it?
   - A. his friend Ryan
   - B. a store clerk
   - C. **his friend Jeff**
   - D. his sister's boyfriend
8. Why did he pay cash at NAPA?
   - A. because he forgot his checkbook
   - B. **because his credit card was expired**
   - C. because his sister had given him cash
9. What time did the man say he stopped at Arby's?
   - A. 11:30
   - B. **12:30**
   - C. 1:30
10. What time did he go to campus?
    - A. 12:30
    - B. **1:00**
    - C. 2:00
11. Why did he have to go back to his sister's place?
    - A. he forgot to turn the oven off
    - B. **he forgot to leave his sister's keys**
    - C. he forgot his bookbag
12. Why was there no one at the computer lab meeting he went to?
    - A. because the meeting was earlier in the day
    - B. because the meeting was cancelled
    - C. **because the meeting was the next day**
13. What did he do in the computer lab when he discovered that there was no group meeting?
    - A. **he checked internet airline fares**
    - B. he chatted with a friend
    - C. he printed a report for class
14. How late was he in the computer lab?
    - A. 1:30
    - B. 2:00
    - C. **2:30**
15. What was on his car when his friend called?
    - A. his friend
    - B. an advertisement
    - C. **a parking ticket**

APPENDIX E

Recall Scores as a Function of Timing and Type of Alibi Information

Mean Overall Recall

	Type of Alibi Information			
Timing of Information	Alibi	Alibi/Guilty	Alibi/Innocent	Total
Before viewing video	48.6 (18.0)	48.3 (16.7)	48.9 (15.4)	48.6 (16.6)
After viewing video	46.7 (19.4)	46.6 (18.5)	53.2 (15.2)	48.8 (17.9)
After primary measures	46.5 (17.7)			
Total	47.3 (18.4)	47.4 (17.6)	51.05 (15.3)	

Mean Frequency of Perfect Recall

Timing of Information	Alibi	Alibi/Guilty	Alibi/Innocent	Total
Before viewing video	17.9 (8.8)	17.5 (8.0)	18.2 (7.1)	17.9 (7.9)
After viewing video	17.6 (9.3)	17.2 (8.1)	19.9 (7.8)	18.2 (8.4)
After primary measures	17.4 (7.9)			
Total	17.6 (8.7)	17.3 (8.0)	19.0 (7.5)	

Mean Frequency of Gist Recall

Timing of Information	Alibi	Alibi/Guilty	Alibi/Innocent	Total
Before viewing video	12.8 (4.4)	13.1 (4.9)	12.5 (4.9)	12.8 (4.7)
After viewing video	11.5 (3.9)	12.2 (4.8)	13.4 (4.2)	12.4 (4.4)
After primary measures	11.9 (4.2)			
Total	12.1 (4.2)	12.6 (4.9)	13.0 (4.5)	

Note: Standard deviations are given in parentheses.

APPENDIX F

Correlations Among Belief Measures and Individual Trait Ratings, Experiment 2

	Main Measures		
	Belief	Detective Belief	Alibi Strength
Relevant Traits			
Conniving	-0.13*	-0.14*	-0.06
Calculating	0.03	0.05	0.10
Deceitful	-0.27*	-0.28*	-0.22*
Suspicious	-0.32*	-0.34*	-0.29*
Cunning	-0.14*	-0.14*	-0.07
Scheming	-0.27*	-0.27*	-0.20*
Honest	0.65*	0.63*	0.57*
Open	0.41*	0.39*	0.43*
Sincere	0.53*	0.52*	0.48*
Trustworthy	0.37*	0.44*	0.37*
Loyal	0.43*	0.45*	0.35*
Intelligent	0.43*	0.46*	0.46*
Irrelevant Traits			
Shrewd	-0.08	-0.11	-0.06
Shy	0.04	0.12*	0.13*
Content	0.22*	0.26*	0.26*
Caring	0.24*	0.25*	0.25*
Curious	0.19*	0.16*	0.20*
Friendly	0.18*	0.17*	0.17*
Ambitious	0.10	0.14*	0.13*
Funny	0.14*	0.17*	0.19*

Note: Correlations marked with * are significant at $p < .05$.

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